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Summary and Outlook for 9th International Symposium on Heavy Flavor Physics ***Helen Quinn***Stanford Linear Accelerator Center**Stanford University, Menlo Park, California 94025***Abstract**

This is the summary talk of a meeting held at the California Institute of Technology Sept 10–13, 2001. I do not attempt to summarize all the beautiful experimental results we have seen this week, nor to repeat the lively theoretical discussions that have occurred. Rather I will present my own biased perspective on what we have learned, and on the important tasks that need our attention as we work to make the most of the rapidly accumulating data in this field.

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INTRODUCTION

I want to begin by thanking our hosts for presenting a well-organized and very effective meeting in a week where such tasks were not easy. Given the dreadful events of last Tuesday we were all a little distracted. None of us will forget where we were in this week, nor that we were well taken care of here. Despite the disaster (which, as one small side-effect, shut down CalTech for the day) a new location for our meeting on Tuesday was found, new arrangements to feed us made quickly and smoothly, and our meeting carried on. The technology continued to function, the talks were all given. We all appreciate the excellent hospitality we were given throughout the week, and the support we are now offered as we all try to figure out how we will get home. We owe a vote of thanks to David Hitlin, Frank Porter, Gregory Dubois-Felsmann, and Anders Ryd and their support staff for their efforts. They made it look easy, but I am sure it was not.

To turn to the physics, I start with a personal comment. I have greatly enjoyed being part of the process of development of the B factory and the BaBar experiment almost from the beginning. This was a fascinating and humbling experience for a theorist. I have seen how much the naive first estimates of what such a facility can achieve are transformed as the reality of building an actual experiment and analyzing real data take over from the back-of-the-envelope guesses with which we begin the process. On both the theory side and the experimental side we (and from here on we means not just BaBar collaborators but all working in this field) have learned where our first approximations are insufficient. The process could be disheartening, except that there are enough clever and determined people involved that somehow we continue to make progress. Despite the difficulties, we manage to maintain optimism and work hard enough that real progress is made. The results on both the theory side and the experimental side that we have seen this week are a proof of that. The job is far from done, but it is well begun. We have begun to unravel the physics of B meson decays.

I mention BaBar first only because it is there that I sit and so I have seen the process there first hand. The same hard work and persistence in other laboratories has also produced new and interesting results reported this summer. We have heard many results this week (even a few actually new ones) and over this last summer. In B physics CLEO, Belle and BaBar are reporting results on some branching ratios as small as 10^{-6} [1], and both BaBar and Belle have reported evidence that the CP violating parameter $\sin(2\beta)$ (or $\sin(2\phi_1)$ which is the same thing) is non-zero [2]. With current accuracy, the result is consistent with the range predicted by the Standard Model. Results on charm, strange and tau physics have also been reported here. I do not intend to summarize all these results, they speak for themselves in the many excellent presentations we have heard [3]. Ongoing experiments will bring more new results and we can expect improved precision on interesting measurements for some time to come.

So what have we learned from all these results? What more do we expect to learn by pursuing the physics of flavor over the next few years? We certainly have learned that persistence pays, and indeed is required to reach our goals. Both in theory and in experiment we must continue to work hard, for several more years at least, to truly pin down all the details of this sector. We have seen that in B -physics as in previous flavor physics there is no free lunch. The golden channel of ψK_s and its cousins ($b \rightarrow c\bar{c}s$ decays of B_d) is interesting. We were not lucky enough to find any challenge to the Standard Model in this first observation of CP violation in the neutral B meson decays. Since one of our major motivations for pursuing this physics is to test the Standard Model story on CP violation that simply means that we must continue with the rest of the program, implementing physics analyses of many more channels.

In our struggle to understand the physics of flavor and with it the physics of CP violation, for at least within the Standard Model the two are intimately linked, we have, once again, run into the fact that we cannot isolate quarks. This means that to study their decays we must inevitably also study some aspects of strong interaction hadronic physics. There are few “golden channels” in which the physics of interest to us for testing the Standard Model can be cleanly separated from the hadronic physics. Our calculational tools are then in need of help. The theory work advances steadily, but the net result is that the predictions we need to be able to make depend on quantities that we cannot directly calculate. We must therefore take a multichannel approach, where theory and experiment feed information to one another.

Theoretical predictions, once hadronic physics enters the picture, depend on some inputs that can only be obtained from measurement or models. Interpretation of measurements depend on input from theory. The process of learning is murky and iterative. In the rest of this talk I will illustrate this with some examples from the theory talks we have heard this week. I will draw a few morals about the way this game must be played if it is to succeed. Being an equal opportunity moralist I have advice for theorists and for experimentalists alike. In the end it comes down to the same advice—a plea for logical clarity and honest revelations about what is an input and what is an output in presenting any theory work or any experimental result. I will make myself clearer on this after the examples. I am going to say a lot of obvious things in this talk, at the risk of sounding preachy and naive. The reason I do this is because it seems sometimes that our tools become so sophisticated that we forget some obvious things. I guess I’m old enough to get away with being preachy on occasion.

My first example is the measurement of charmless semileptonic B decays and the extraction of V_{ub} . Here we heard of new theoretical work from Christain Bauer [4] and ideas on using the spectrum of $B \rightarrow s\gamma$ data as a way to measure some input parameters from Ira Rothstein [5]. The CLEO experiment has begun to implement some of these ideas [6]. You all know the problem. To extract V_{ub} we must measure

charmless decays. The cuts on the data that must be introduced to remove the background from the much more common decays to charm bring with them a price for the theorists. The fraction of the data kept after any cuts must be calculated. This fraction is sensitive to details of the spectrum that may not be reliably predicted by a quark level calculation.

Christian Bauer showed us how a combination of cuts can be used to limit the sensitivity to this problem and suggested how one can also gain some tests of that sensitivity by varying the cut prescription. He advised minimizing sensitivity to theory. That is impossible; we are trying to extract a theory parameter. But what he really meant was minimizing sensitivity to the uncertainties in the theory that arise from soft physics. That can be done! I would further suggest that the data should be presented in two ways. The cuts may be tuned based on theory input, but the result should first be stated in as theory independent a fashion as possible. Only after that should the analysis that gives the theoretical parameter be introduced. In this case this separation is readily made. One has simply to quote a rate of events in a given kinematic region, or perhaps a table of such numbers for different choices of kinematic cuts. The second step, of turning that table into a best estimate for V_{ub} , is also needed. It should be kept separate. In this step theory and experiment become inextricably mixed together. The reason I plead for the first step, the presentation of data with no theory in the numbers, is that that is what will allow us to come back at a later date (even possibly after the collaboration presenting the results is long disbanded) and re-analyze the data with new theory inputs. Experiments should not become so theory-driven that they only present results for theoretically interesting quantities. I know there are cases where my advice is simply too naive, where there is essentially no way to present a theory-free set of numbers. It is just this fact that has led us into the bad habit of confounding the two steps, that of measurement and that of interpretation. The pattern is perhaps reinforced when experiments publish only as letters; there is not room in a letter for the two steps, and the second number is regarded as the real result of the measurement. In the short term this is true, in the long term the theory independent result has more staying power! My advice to experiments is to pay attention to whether or not they are forced into this procrustean bed, or whether they are allowing themselves to fall there simply because they have not tried to hard enough avoid it!

I want to return to the idea that Ira Rothstein [5] talked about, that of using one set of channels to measure some soft physics parameters and then using those parameters in interpreting another set of channels. This may sound obvious, it is in practice neither obvious nor simple to implement. The issue is that these parameters are not physical quantities in the technical sense—they are definition and convention dependent. This means that one must in fact do some higher order QCD calculation to understand how the spectrum in say radiative B decays is related to that in semileptonic B decays. This is ongoing work, and the “best” or even standard con-

ventions for defining the relevant B physics parameters may not yet have emerged. This is a technical point, not a problem. However when experiments quote numbers for a theoretically-defined quantity, such as the parameter $\overline{\Lambda}$ of the heavy quark effective theory, they must at the same time tell us what definition of this parameter (or equivalently, in this case, of quark mass) they are using. Without such a definition the quantity is meaningless.

A second topic where we saw some interesting theoretical analysis was on the subject of $D\overline{D}$ mixing, long advertized by theorists as a good place to look for new physics effects. In the talk by Zoltan Ligeti [7] we heard how the naively predicted pattern of the operator product expansion contributions to x and y is possibly disrupted because of $SU(3)$ breaking effects. Since the otherwise-leading operator is suppressed by $SU(3)$ symmetry, the symmetry-breaking effects can be significant. Indeed they may alter the expected relative sizes of x and y . The size of expected Standard Model effects is also enhanced once $SU(3)$ breaking is considered. So the lesson here is not to trust first rough estimates of theoretical uncertainties, these things always some study. However the effects here are still at most at the few percent level, so this remains a place to look for new physics effects.

No method can completely remove the issues of theoretical uncertainties. No matter what technical improvements are made, in the end we must rely on some quark-level calculations and some version of quark-hadron duality. Now it is the job of the theorists to estimate how big the remaining uncertainties could be. Unfortunately there is often no rigorous approach that gives a definite answer. I believe that theorists must give skeptical answers here, but that it is important to try to be quantitative. It is just as bad to give an off-the-cuff overestimate of the uncertainties as it is to claim that everything is under control with an optimistic estimate of uncertainties.

So let us spend a little time considering how big can the violations of quark hadron duality be? What sets the scale of these effects? What level of averaging is needed to remove the detailed dependence on resonance masses in a spectrum and hence to get results that depend only on the underlying quark diagrams? To make this point I will digress a little and talk about a case where I know something about the answers, that is the rate of hadron production in e^+e^- annihilation. This is typically expressed as a ratio $R_{e^+e^-}$ of hadron to non-resonant muon-pair production [8]. One can prove quite formally that an integral over this ratio along the entire real physical cut can be given by the relevant quark-loop graph calculated in the deep Euclidean region. Indeed one can use the analytic properties of the quark graph to calculate an integral that weights a small segment of the cut. In the Euclidean region there are no small denominators from near-mass shell propagators and so the power series expansion is well behaved.

What limits the reliability of the calculation is that as one approaches a threshold on the physical cut certain propagators in the diagram give small denominator factors. We know which diagrams have to be summed to all orders to produce the onium

resonances just below a new quark threshold. Right on resonance the violations of duality are huge if we do not make this resummation. We can avoid the resummation only if we calculate an integral that can be determined without the knowledge of the function close to the threshold. That is the essence of the “global” duality –it gives us a correct averaged cross-section, but not the detailed threshold and sub-threshold structure. On the other hand, once we are well above threshold we can approach quite close to the physical cut without any small denominators appearing in any diagram. Indeed we find that the averaged rate and the naive “local” quark calculation agree well in such a region. There are two main points that this example teaches us. The first is that the violations of local duality are very large if we are foolish enough to try to use it where some quark propagator goes on-shell, but quite small when we are far from any threshold. In a properly averaged quantity, the “duality violations” are small. The second point is that a careful enough examination of the quark diagrams revealed what went wrong with the perturbation series at the threshold, and hence indicated the range over which averaging was needed to avoid these problems. The size of duality violations is then controlled by the averaging scale compared to Λ_{QCD} (and also to light quark masses which are smaller).

These features then generalize to two questions. The first is how much do we need to average over hadronic mass spectra to ensure that corrections to a quark-hadron duality estimate are under control? The second is to find what effects can give large violations for the process in question, and what sets the scale of these effects in a suitably averaged quantity. In an ideal world we could give rigorous answers to these questions, in the real world different theorists reach different conclusions. For example for the case of the inclusive semileptonic decay rate to particle with charm, where no significant kinematic cuts are required to select events, these corrections have been discussed in some detail and are generally agreed to be small. The range of lepton momenta achieves some averaging over the hadron mass spectrum. The argument among theorists is then over whether small means a few percent or of order 10^{-3} . Bigi and Uraltsev have argued that the latter number is appropriate [9]. Their argument is plausible; no one has shown specific effects which they have ignored. However this same type of argumentation leads to other results that are discrepant with experiment, for example the differences in the lifetimes of different b -containing mesons and baryons are larger than duality-based arguments would predict. The conundrum is then whether we are at the stage where we should say these are serious violations of Standard Model predictions, or simply that violations of quark-hadron duality are bigger than expected. Any skeptical observer takes the latter position. But the results certainly show how important it is to try to gain a better understanding of how to quantify such duality violations reliably. We may lose the ability to recognize many signals of new physics if we do not do better on this front. Theorists generally divide into those who have made the calculations and are willing to make estimates of the size of the breaking, and skeptics who say we cannot reliably estimate these

effects. The skeptics typically cannot point to any specific error in the calculations that have been made. I admit that I have generally been among the latter group—but at this stage of the game it is an inadequate position to take. We theorists need to keep attempting to do better.

However, at the same time as I preach that skeptical theorists should take on this challenge, I warn the experimentalists that the most aggressive (optimistic) estimates of theoretical uncertainties are often not reliable. The problem is that the diseases of the quark-level perturbation theory calculation can be very subtle and do not always manifest themselves in an obvious way. For the case of B decays, we will only learn how to limit the size of duality violating effects by comparing predictions with data in many, many channels. In particular it is important to test the sensitivity of results to variation of any experimental cuts or changes in procedure. When there is significant sensitivity in the parameter extraction to cut-variation or to input models or assumptions then we know that we cannot trust the duality-based calculation. However, even when the sensitivity to variation in input is small we still cannot be certain that the effects of duality violations are small. This all leaves a very murky path to finding a distinction between new physics effects and unexpectedly large corrections to our calculations. I hope and expect that we will be able to find ways to separate these two things, but there are no guarantees.

One class of processes for which this process is now at a well-developed stage is the calculation of two-pseudo-scalar decay channels using qcd-improved factorization or the alternative formulation called perturbative qcd. We have all seen the papers and heard talks by two groups working on this topic. It has taken me some time to understand some of the details of this work. The results of the two groups are quite different, but it appears at first glance that they are pursuing the same methods. The methods both keep leading order terms in Λ_{QCD}/m_b and calculate the leading $\alpha_s(m_b)$ corrections. The groups get quite different results on some points. The problem is that even the conclusions on the power counting for the relative sizes of certain contributions are dependent on assumptions about the quark distribution function end-point behavior. In my opinion the fact that two independent groups have been working on this is important. We have seen that the estimates of theoretical uncertainties of one group have evolved due to the work of the other group. This is what we need, honest efforts to give good error estimates, and, to find what parts of those estimates are sensitive to assumptions, more than one group doing independent work on the same problem. This then leads to an iterative process that can eventually give us some confidence that we have a good estimate of theoretical uncertainties.

The group of Beneke *et al.* [10], represented in this meeting in the talk by Matthias Neubert, assume that the end point behavior is a power of x , and do not include any Sudakov suppression factor. They argue this effect does not play a significant role at the actual B mass scale. They then use data and models to give the needed input for matrix elements; for example, the transition matrix element for B to pseudoscalar

meson is taken from semileptonic decay measurements. They make an honest effort to test a range of assumptions, for example for quark distribution functions, and see how their results vary as those input assumption are varied. This is the model we all must follow. But no matter how honest a group might be, it is important to also have another group or groups think independently about what input assumptions are reasonable. Only in this way can there be a serious discussion about whether the estimates of theory uncertainties are conservative or aggressive.

The group of Keum *et al.* [11], represented here by the talk by Y-Y. Keum, make a different set of assumptions. They include transverse-momentum dependence via Sudakov factor in the quark distribution functions. This suppresses the contribution which is the leading term in the Beneke *et al.* approach, so that in this so-called perturbative QCD approach the dominant term is an order $\alpha_s(m_b)$ hard-gluon exchange to the spectator quark. Then the inputs are light-cone quark distribution functions for the mesons, which they argue are calculable. I find that last statement quite doubtful (see the arguments on this subject in a paper that appeared shortly after this talk [12]). However one can simply regard these inputs as an alternate (and stronger) set of assumptions. One then must ask whether these are reasonable assumptions. They choose the parameters of the Sudakov term so that the scale of average transverse momenta in the quark distributions is $k_\perp^2 = m_b \Lambda_{QCD}$. This is an incorrect choice! Even for the B meson, and certainly for the pion, the transverse momenta should be scaled only by Λ_{QCD} . This choice has significant implications for their numerical results, which I therefore find suspect.

However even if their results are not yet reliable, there are some points that have been raised by these authors that have led to some revisions of the error estimates of the former group. This reinforces my statement that it is always useful to have two groups approaching the problem independently, to test assumptions about theoretical uncertainties. The major issue raised by the second set of calculations is the impact of, and control over, the (Λ_{QCD}/m_b) -suppressed contributions. The contribution of annihilation graphs is one such term. In the Beneke *et al.* formalism it turns out that this contribution is infra-red singular and hence dependent on the cut-offs introduced to control this singularity. Recent papers of this group include an larger estimate of the uncertainties due to these terms than earlier papers (as far as I can tell). In the Keum *et al.* approach the infrared behavior of this contribution is softened by the k_\perp dependence of quark propagators, and the term is found to be numerically significant. In particular it contributes to a large imaginary part, the size of which is important for any estimate of direct CP -violation. These numerical results depend on the incorrect scale for the average k_\perp mentioned above, and so I do not trust them. However the fact that this contribution needs to be estimated carefully is an important point that was raised by this work. The public debate between the two groups has also helped those not directly involved to learn what are the critical issues. That is an important impact of having two sets of calculations.

This example provides a model for our path into the future. It is not possible to avoid all hadronic physics questions. We need theorists to tackle them and to make some assumptions in order to do so. We then must do a serious job of thinking about the range of input assumptions. Human nature being what it is, we must have more than one group tackle that job. That helps ensure a public discussion of all relevant issues. However much this is done, there will always be some remaining tension between those who have made the estimates and skeptics who doubt their methods. The history of the $\Delta I = 1/2$ enhancement and $\frac{\epsilon'}{\epsilon}$ provide a warning that favors the skeptics, but B -decays offer a new regime where more systematic expansions are available. There is ongoing progress on the theory of this regime.

So this effort must continue, both on the theory front in estimating uncertainties, and on the experimental front in presenting measurements in as theory-independent a fashion as possible. Then the clever application of theory to experimental data is needed, to provide the cleanest possible tests of the Standard Model. That requires close cooperation of theorists and experimentalists. Even after all the hard work is done the question remains whether we can be confident enough of our uncertainty estimates that we can be sure a discrepant measurement tells us there is new physics. The pessimistic view is that we will always be able to adjust the theory uncertainties to cover any measured results. The optimistic view is that eventually we will gain enough confidence in our methods to recognize true discrepancies if such exist. I suspect that any one result will not be convincing. It will take a pattern of discrepancies in fitting many channels to tell us that the Standard Model is failing. Both theorists and experiments still have much work to do. I have been impressed up till now by the persistence on both fronts in this work. This persistence has yielded steady progress, as we have seen in this meeting. I am hopeful that this progress will continue.

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